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Dr. Paul L. Hoch
1525 Acton Street
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Dear Dr. Hoch:

This responds to your long letter of 08/18/82.

I don't know what the House "experts" were looking at. I do know that they weren't looking at a portion recorded at the time of the shots. I know also that their analysis was a good idea carried out with less than adequate care and skepticism.

There is really no reason to compare the probability of the hypothesis with the probability that the recording was due to random noise. That is the easiest comparison to make, but there is no reason a priori (and certainly none from observation) to characterize the noise on the recordings as "random." In order to compare with any other hypothesis, one would have to assume some "burstiness" about the noise, or to derive that from observations of the recording. The recording is anything but "stationary" so that would not be an easy job.

It is by avoiding such experimental designs that I have accomplished what I have in physics; I'm not about to start analyzing somebody else's poor experiment (which we have disproved on other grounds) to find out how to do the tedious and complex analysis demanded by a poor experiment. For your information, I enclose the paper I gave to the Fifth Cambridge Conference on Relativity at MIT, dealing with Joe Weber's results.

As for your question "On what grounds was the Committee initially negative about the BBN/W&A analysis?" I don't characterize the Committee's view in that way. In fact, I don't think the Committee had a view until it wrote its final report. I personally thought the idea of matching echo patterns was quite clever, in view of my experience in acoustic location of artillery, and it was not until I had read the reports and then tried to understand from those reports and from the conversations with Barger and W&A that I recognized their case was really very weak. In fact, my earliest substantial concern arose from the fact that BBN and W&A initially found different regions of the recording as representing the third "shot." So I do not accept your premise.

As to "what I really think of the FBI's rebuttal to the House experts," I think they also went astray. They tried to match the patterns with another pattern of assuredly non-random noise, but they made a faulty analysis of the statistics!

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*Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

I can't reply to every statement or question in your letter, but I did note your sentence "If I am wrong in concluding that the NRC Committee deliberately minimized the weaknesses of the FBI analysis, please correct me." You are wrong. The FBI report was irrelevant to our work; had we been asked to analyze the FBI report, we would have put a lot more effort on it and given it more space in the report.

As for how thoroughly we looked for evidence "not in the BBN/W&A reports which would support that conclusions," I personally certainly kept my eyes open for everything I could see. If I had thought or anyone in the community had informed me of any acoustic evidence which might support the BBN/W&A conclusions, I would have looked at it.

As for the "hypothesis that the recording has been copied and changed," I thought about it and looked at it, but I can't go beyond the conclusions and analysis stated in the report. I do think that we did pretty well in obtaining a copy of Channel II without "repeats," which made possible the improved timing analyses.

Within a few weeks we will put into the Academy file a more technical report of the work done by me and my associates at IBM Yorktown Heights. That report will of course be available to the public, including you.

You understand that the Committee on Ballistic Acoustics was not asked to review the question of conspiracy, whether Oswald really shot Kennedy, whether Oswald was a paid agent, or any of those questions. Of course, had we found answers we would have reported them, but we did not. We did establish in several ways that the portion of the recording in question was made long after the actual shots, so that even if the analysis of "95% probability" were valid, that hypothesis would have to be rejected and one would have again to admit that a 5% probability comes up once in 20 times! Actually, we did enough work on the probabilities to show that the "95%" figure was unwarranted, although not enough (I don't know that anybody could do a proper analysis) to know what the best figure is.

I hope this reply is of some use to you.

Sincerely yours,

Richard L. Garwin rsa

Richard L. Garwin

Encl:

06/10/74 "The Evidence for Detection of Kilohertz Gravitational Radiation," presented at the "Fifth Cambridge" Conference on Relativity (MIT). (061074EDKG)

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